

THE
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GENERAL REVIEWS AND SUMMARIES

PATHOPSYCHOLOGY AND PSYCHOPATHOLOGY

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I

The last year has brought to us the emphasis of a new contrast in our field, viz., the opposition of pathopsychology and psychopathology.

A mere creation of contrasts not infrequently helps in bringing otherwise vague perspectives to clearer attention, especially so in the discussions of broader issues. Often enough the generalities are of a kind which need not touch the genuine worker who can readily leave them to the time when enough facts are available to make a conclusion easy, and who in the meantime trusts the sound trend of the day or the vogue of the programs of our scientific societies. At the same time, if a beginner or the average worker *has* the chance to grasp clear starting points and perspectives, he is bound to be better off than if he moves in a groove made by others or yields to temptations which may unnecessarily check or side-track his soundest instincts of reaction to the world of facts. And where a branch of science is just beginning to shape itself, the neighbors—in our special case, the psychologist and the physician—will be better able to appreciate the attitude of new departments in a measure as there is clearness about the starting-point and perspectives; for to take success as the only test merely opens the door to the deplorable flood of uncritical psychopathologies which struggle for the supremacy in the public eye, not without effects upon the more scientifically minded workers.

In previous numbers of the BULLETIN an effort has been made to review especially those topics which promised to have an influence upon the geographical and political map of systematized science and what we might call the interstate relations: psychopathology as nosology and as non-dogmatic pathology in 1904; then the evolution of a possible dynamic standpoint; the development of the association-experiment, and of the interpretation of cerebral integration in aphasia and apraxia; the psychological experiment in psychopathology, and more special problems, such as the feeling of reality (1905); aphasia, and the relation of emotional and intellectual functions in paranoia and obsessions, and the psychopathological development in association studies (1906); next the psychogenic factors in the development of psychoses, and misconceptions of a dogmatic "medical psychology" and the "revisions" of aphasia (1907); and finally the criticism of nosology and mental dynamics in 1908, and in 1910 the Freudian psychology. These were the leading topics discussed by the contributors of the psychopathological numbers of the BULLETIN. Today we are confronted with a double current manifested in the assertion of autonomy of introspective psychology and also the creation of a contrast of pathopsychology and psychopathology, which may or may not complicate the already complex path of the domain of concern to us. The latter comes out most strongly in the new *Zeitschrift für Pathopsychologie*, edited by Wilhelm Specht; and I also wish to discuss in this connection some reflections on the kindred current in recent psychology, as far as it is apt to influence the worker in mental disorders.

II

In his preface, Specht urges that the way from psychiatry to psychology must necessarily pass through philosophy. The chief factor of retardation of psychiatry is the materialistic dogma of the epiphenomenal nature of "psychics" (des Psychischen). Psychiatry must learn to surrender its one-sided focusing upon the brain and must learn to apply psychological methods to mental diseases. On the other hand psychology must accept the wealth of opportunities and the kind of broadening out which, in its way, pathology has given to physiology. English and French philosophy (Maudsley, Taine and Ribot, and lately also Bergson) has long appreciated the importance of pathology for psychology; Störing and Oesterreich and others have created psychological studies in pathology in Germany; a few alienists have at least developed a psychologically

more refined symptomatology, and others promoted a deeper psychological understanding of diseases. But "in psychiatric circles not even the essential difference between mere clinical experimental psychology and pathopsychology is clearly grasped." To them to speak of function is merely an admission of insufficient anatomical knowledge. Thus it happens that a reduction of the facts to terms of function in a truly psychological sense is only in its beginning. With his *Zeitschrift* he wishes to give the study of mental diseases a new foundation by bringing the psychologist and psychiatrist together in the work of pathology of mental life on a really psychological foundation, in a pathopsychology, dealing with the pathology of the *individual consciousness*, and also with the psychology of abnormal conditions and creations of *society*.

On pp. 4-49 Specht gives a full discussion of the program of pathopsychology. He shows how a careful description and analysis of the pathological phenomena in mental diseases are possible only through penetration into the mental mechanism of the disorders.

Without wanting to discuss why, with all its productivity, "psychology as developed under the leadership of Wundt does not seem to succeed in establishing theses which would be teachable and generally acceptable," and without taking sides in the recent disputes concerning the experimental method, Specht wishes to show first wherein the pathological method is preferable to the experimental method. He concedes to such a philosophical critic as Husserl (*Logos*, 1, Heft 3, 1910-11) a whole range of problems (the essence of psychics, the nature of our understanding other minds, the relation of psychics to a self, the question of the independent existence of thought or of degree of consciousness, etc.); they cannot, he thinks, be settled by observation or experimentation; "they belong to the epistemology of psychics and precede inductive psychology as philosophical propædeutics of psychology or as the phenomenology of psychics"; but he finds an ample field for the experimental method in the search for lawfulness wherever the task is one of the *inductive* science of psychology, whether it does or does not turn its attention especially upon the introspective issues.

The aim of all experimentally modified introspection is to single out or to eliminate certain components. In the normal, this is only approximately possible; perceptive and recognitive functions cannot be dissociated in the normal; nor can a somatic memory of Bergson be completely cut off from the representative memory in the normal. Pathology however furnishes exactly such dissociations. Only

pathology knows of states in which a hand although anæsthetic may nevertheless recognize an object; or in which recognition may be eliminated while perception and memory are preserved; or in which objects given to external perception give up their claim to be present; or in which the realization of a motor intention becomes dependent upon actual concepts or memories of motion; or in which (as in hallucinations) the meanings are no longer founded on sensory contents, but where an already prepared intention of meaning seems to slip in between sensation and content. Normal psychology can at best come near such an elimination, or solution of continuity, or perversion of functions, but it never can realize them completely. For some reason Specht is however exceedingly cautious about his applications and somewhat arbitrary. "There follows from this a negative and a positive rule concerning the significance of pathology for psychological science. Wherever there are phenomena which only occur in pathological conditions, an immediate application of pathology to psychology is not admissible." Hallucinations for instance are considered as strictly pathological phenomena, devoid of all transition through illusion to normal perception. "Similarly it is pathological when volitional intention requires motor images for the realization of its content." The phenomenon of the positive after-image has been too rashly used for false theories of sense-perception and even for metaphysical theories on the subjectivity of the contents of perception. The association psychology should not appeal directly to the findings in mania and in intoxication; and the doctrine of the mind-substance should not be considered as refuted by the dissociation of the ego in hysteria, lest one ignore the fact that these conditions are abnormal and "that there are also occasional thinking, non-intoxicated and non-hysterical personalities." The physiologist would err in a similar manner if he described the elimination of albumen as a function of the kidney and not merely a function of the diseased kidney.

There is no doubt that Specht is justified in drawing the attention to the necessity of reserve. It is necessary to check the uncritical and to cultivate a certain respect for the concrete situation of any special type of occurrence as opposed to the license of untrammelled generalization. Passing to the positive rule, Specht shows how the perception of things by an anæsthetic hand endorses Külpe's rehabilitation of the inner sense and how it warns against sensualistic generalization which minimizes the difference between recognizing the *thing* and recognizing the tactile sensations. Or (p. 14) he shows

how, notwithstanding preserved capacity of perception and of *remembrance* of previous perception, recognition can be lacking; and that abolition of visual memory does not entail mind-blindness, so that Bergson's contrast of memory for motor utilizations and of independent memories is more justified than the current explanations such as those of Lehmann (*Phil. Stud.*, Vol. V.). This does not call for a wholesale transfer of conceptions from pathopsychology to psychology; it is a broadening of the world of facts on which general theories should be brought to a test. In my own mode of expression I should have to emphasize the *respect for the concrete situation* and the mistrust of any "absolute" generalization which would want to be anything more than a more or less comprehensive simplification of our concrete picture of the world and our experience. If we bear this in mind, some of the points specified above as warnings may not have to be brushed aside in as final a fashion as Specht seems to feel obliged to do, as with regard to the hallucinations and the artificial reproduction of "flight of ideas," etc. They, like the *favoured* instances of acceptable generalization, will stand or fall according to whether they will stand the tests of repeated experimentation and analytical penetration.

Pathology adds to our knowledge not only by eliminating certain connections, but by the independent variation of various functions (or, as I should put it, of various integrated factors). It does for us what caricature and the experiment do, and it suggests new channels of work and new viewpoints. Specht certainly makes plain "the value of pathology as shown by elimination of functions" and "the narrowness of the thesis that normal mental life should not be explained from the pathological side."

Specht (p. 16) next passes to the question of *what rôle can pathopsychology play in the business of psychiatry* (which is the "discrimination and cure of mental diseases"). With the dogmatic assurance shared by Münsterberg, Specht claims that medicine ceases to be a medical *science*, if it ceases to prove that a patient asking for treatment has this or that *disease*; the knowledge of the "disease" and the knowledge of the causes calls for the baths, medicaments, psychotherapy, etc. Psychiatry thus must make it its business to recognize and cure mental *diseases*.

Specht accepts the establishment of absolutely distinct disease entities as achieved (?), and also the demonstration of some definite etiologies; but he deplores the hopelessness and dogmatic pessimism which spurns the psychological concepts even in the functional dis-

eases and there can reckon merely with "brain diseases in which we are still ignorant of the lesion," and which is apt to see only in these "physical" processes realities, even though they may be merely hypothetical, while the psychic facts figure as mere epiphenomena; so that the psychiatrist maintains a passive attitude wherever he does not find a point of attack upon the gross or molecular changes in the brain. In the mind of the "psychiatrist" even mental influences can have a beneficial effect only through the influence on the molecular conditions with a secondary effect on the mental state (Specht illustrates this position by Kraepelin's attitude). In contrast to this, Specht wants to restore their reality to the psychic data, and he does not want to wait for the day when a drug will be available to counteract a melancholia as a drug counteracts constipation; he wants to recognize mental diseases as mental diseases and study them for *psychical* causes (if they exist) and a corresponding therapy, or for physical causes when physical causes exist; and he wants to distinguish brain diseases and mental diseases (rather than "organic" and "functional" diseases). "Brain diseases" and "mental diseases" are not altogether synonymous with "exogenous" and "endogenous" disorders; but in the one, therapy attacks the *brain*, in the other it attacks the *mind* (in which I should emphasize conduct and behavior). Only experience and the facts will decide which conditions belong to the one and which to the other group. While nobody would exclude the possibility that some day a drug might affect the brain in exactly the necessary way to bring about even the mental changes required, a study of the psychogenic mental disorder from the mental side in the meantime is to say the least absolutely justified, even if, as in dementia præcox, the deterioration speaks definitely for an involvement of the brain tissue in the decline.

Lack of space forces me to give only a limited summary of the well written arguments of pp. 16-49. They form a counterpart to my own statements in earlier years of this BULLETIN. Specht gives a very lucid discussion of the necessity of a more plastic and functional conception of "disposition" which makes different persons react differently to the same difficulty (as, *e. g.*, different women would react differently to marital infelicity), and he formulates the treatment as an attempt to make the patient see things in a wholesome light—not through blunt arguments but through helps which help. He appeals to the psychiatrist to approach his work free from all dogmatic presupposition and to depend on what he finds by experience, to recognize psychogenic and non-psychogenic disturbances and with

this the existence of mental causes, seen both in the production and in the remedial modification of disease. It is quite characteristic that he harks back to some psychiatric voices of sixty years ago, before the anatomical fascination created the doctrine of exclusive salvation in putting all psychopathology in terms of hypothetical brain-changes.

Specht errs if he claims that Freud's somewhat over-systematized psychopathology is the only attempt in this direction in modern psychiatry. He is not informed of the work in the *PSYCHOLOGICAL BULLETIN*. And I must confess that the recent discussion of the introduction of psychology in the medical curriculum shows a broader and freer development than the one under which Specht feels justified to introduce his new journal. He certainly does not make clear *how* he promises help to the psychiatrist who should find his way to psychology "through philosophy." Will that become clear through his philosophical contributor?

III

Münsterberg ("Psychologie und Pathologie," pp. 50-66) takes up the methodological issues. He complains of the continual careless intermingling of the two expressions, psychopathology and pathopsychology (or, he might possibly have said more justly, of the promiscuous use of the word psychopathology where M. would prefer the adoption of the term pathopsychology). An investigation may be of importance to both psychology and to pathology, but it naturally is logically differently focused according to whether it aims to serve a knowledge of the mental phenomena or of the diseases. He peremptorily assumes that *qua* pathology any mental disorder must be viewed as "symptom of some definite disease," while *qua* psychology it is treated as a variation of other similar *mental* variations.

Pathopsychology at once appears under two sets of conditions. Psychology may be furthered in its intrinsic problems by studying the abnormal states and processes beside those of normal life; on the other hand it may draw in pathology (I should say nature's experiments) for the purpose of interpreting normal mental life (I should say mental life in general). "In the one case the relation to pathology yields a special *group of problems*; in the other case a special *method* for psychology. Both result in a gain of purely psychological knowledge and therefore constitute pathopsychology; but they form two different fields of work which coincide only in certain points," very much as the psychology of normal life and experimental

psychology. The same kind of contrasts can be made in animal psychology and in physiological psychology (and M. might say also in pathology itself when it studies constitutions as well as the specially definable processes or "diseases," or when we compare its descriptive-analytical part and the experimental pathology).

X Pathopsychology thus studies the mental processes occurring in disease not as symptoms of diseases but as deviations from the normal course of mental life and for the gain of purely psychological knowledge, partly to broaden the field of facts (a special group of contents) and partly to explain normal mental life (as a counterpart of experimental psychology in a new field, or as a special method). M. urges the verbally plausible contrast between an extension of matter and an extension of method.

By giving up the concept of disease and morbidness, pathopsychology according to M. "encounters considerable methodological difficulties." The contrast of health and disease is logically simple. Health and disease necessarily refer to biological conditions and are concepts borrowed from general pathology, which is based on diseases of the body; a special adaptation to the psychical conditions is therefore not necessary for the concept of "disease." Whatever damages the self-preservation of the organism is morbid whether it shows in the purely physical domain or in part also in the mental field. "Hence the concept of what is pathological" remains absolutely dependent on the vital conditions of the physical body even when we are concerned with psychology, and it does not require any special adaptation out of consideration for psychics. (The reader will see that all this resolves itself to the traditional conception that there are no mental "diseases" but only physical "diseases" and to M.'s views concerning causality.)

In the mental domain proper we can only speak of normality and abnormality, i. e., reference to mere *averages* (which would be lowered in case of increase of insanity) or to an *ideal of harmony* of the mental forces. Abnormality and disease are not parallel concepts: a genius is abnormal but not pathological; from a teleological-psychological standpoint dreams should be looked upon as abnormal and yet not a pathological symptom, but on the contrary, with some psychopathologists, a really important help to the normal organic functioning; whereas the euphoria of the consumptive may be pathologically part of the disease but is psychologically normal. In "suggestion" the abnormal begins "where the hypnotic influence sets in," or in blindness or deafness "where they modify a mental life"; yet neither

of these abnormal states turns on the notion of disease. "Pathopsychology will attempt to explain the whole range of the abnormal with the helps of the normal psychology," and by using its knowledge of memory, of attention, of feelings, of volition. To all this we might say that to the psychologist, as to the scientist generally, normality and abnormality can no longer be a primary scientific issue. Science must generalize the venerable declaration of breadth in "*Nihil humani a me alienum puto*," and it must accept the fact that this broadening out is to be allowed without *a priori* restrictions—for who knows what the investigator may find and what he must be ready to meet. At this point, M. properly criticizes the use of the term "applied psychology" for pathopsychology, which would be as inappropriate as calling the psychology of myths, of morals, and of languages, applied psychology. So much for M.'s discussion of pathopsychology as "psychological penetration of the abnormal psychological phenomena."

M. next takes up pathopsychology as determined by the *methodological* viewpoint. "It is the entire psychology as far as it is furthered by the study of abnormal and especially pathological phenomena," especially where clinical observation gives insight into the psychical mechanism. But M. evidently still assumes that whatever mental phenomena occur in disease must, in pathopsychology, be viewed as an exaggeration or a reduction of the "normal."

Psychopathology is "quite different"; according to M. it turns absolutely on the concept of "disease." As far as it studies special contents, it takes up the special mental symptoms in specifically mental diseases and also in other diseases. As far as it takes the methodological viewpoint, it deals with the entire domain of human diseases as far as its study can be furthered by the consideration of mental facts and psychological knowledge. "Here then the normal mental attitude is the real starting point. We may take as an instance the much-disputed mental tests which try to elucidate a pathological behavior in the domain of a simple measurable activity by comparing it with the normal typical condition," etc.

The four fields are further complicated by the *physiopsychological* and *psychophysiological* correlations and substitutions which must be made problems "clearly kept apart."

M. next turns from the theoretical field to the union of psychology and pathology for practical results in the service of hygiene and pedagogy, in psychodiagnosis and psychoprognois, or in the diagnosis of mental states by non-psychological means, or with the help of

normal-psychological demonstrations (as in the disorders of intelligence). Even for the study of peripheral and spinal diseases psychological methods (tests of sensibility or motility) may be used. Even broader is the field of therapy in the form of "psychiatry" and "psychotherapy." Psychiatry may use baths and non-psychological helps; psychopathology uses mental influences against mental and physical disorders (such as digestive disorders, etc.). As a specially promising category M. suggests the psychological experiment on the normal in the interest of psychology but under the direction of pathology. Kraepelin did the reverse; now should come the turn of the use in the normal of what, for instance, special studies of amnesia have suggested.

"It would, however, be methodological confusion to expect that pathological observation could throw any light on the fundamental questions of psychological conceptions." The experience with suggestion could not possibly further the theory of the relation of mind and body; of a great share (?) of Freudian literature he claims that it is pervaded by the thought that psychoanalysis furnishes evidence of a causal action of psychics "independent of brain-processes," and that for this reason it vainly assumes that the psychophysical parallelism is overcome. It is also futile to expect that one could in any way justify the psychological concept of the unconscious by facts in pathology, or a decision on whether psychological analysis finds merely elements of content or also elements of function, or whether there are various degrees of consciousness, etc. Not one of these questions can even be touched by any pathological study as little as by the normal psychological experiment. All this can be as little a problem within psychology as the problem of time and space can be a problem of physics.

To an active investigator in psychopathology, these discriminations may be pleasing if he agrees with the dogmatic foundations from which Münsterberg chooses to start. If, however, he should have relegated the concept of a "disease" to the category of mere convenient medical logic in nosology, while at bottom in his pathology he only recognizes conditions and factors at work in experiments of nature and reaction-types worth defining (see *PSYCH. BULL.*, 1908, 5, 245-261), he may easily come to feel obligatory nosological assumptions a hindrance or at least an encumbrance of doubtful value, something needing more help itself than it can possibly give in pathology. With a profound respect for the helps of logic and critical definition of problems and epistemology, I do consider it essential

that the first thing to aim at is sufficient accuracy and clearness of work, just as we demand it (together with rigid clearness) as we embark on a bacteriological or chemical analysis *or* a logical elaboration of data. For all this a first-hand familiarity with the facts and with the fundamental methods in use must precede whatever subsequent philosophical consideration one wants to introduce. Only if that is granted does the help and critical training of the philosopher come in good stead; otherwise its contrasts smack of word-play and logomachia.

The psychologist and the pathologist who take up the study of a depression, or of an hysterical repression, or a psychasthenic rumination will do well to ascertain the facts, determine their actual interrelation in a chain of causes and effects (*i. e.*, in terms of an experiment of nature), and then they analyze the relative rôle of each link and the modifiability of the links and of the whole chain—and all this can be done without speaking of “symptoms of a disease” or a discussion of what is “normal.” Fundamentally both the psychologist and the pathologist, if there are such in pure culture, must take all the facts into consideration to be on a strictly scientific basis; the difference will only show in the emphasis on various groups of facts and their interrelation, and in the grouping of the material; and the value of the one or the other emphasis and method can only be an issue of economy and accuracy in the procedure, but not something radically different, unless, of course, we start out with a psychologist who knows of no causality in his realm, and a pathologist who treats psychics by elimination. The very transformation on this point is the fruit of work with facts and the adaptation is far from being aprioristic, as Münsterberg seems to demand. It is after all the facts which call for the making of categories or for their simplification or readjustment, and the preliminary result next may call for logical-philosophical sifting.

The discussion is given so much space here because it makes one long for the passing of aprioristic specialism. First-hand work must more and more become the condition for the whole range of verbal and conceptual permutations of psyche, pathos and logos, and the ramifications will be considered safe or in need of more or less modesty and reserve, according to the extent of a writer's first-hand mastery of work in the integrated fields—biology, physiology, psychology, logic and what not. I feel that I must be in perfect harmony with M. on this point: that it will be considered more and more hazardous and dilettantic to make claims in psychology *or* in pathology without a

fair working knowledge of the working elements of both. As long, however, as the working knowledge is safe, we can trust continued work better than the continual creation of methodological contrasts. Where does the system of logical permutation lead us?

The claim of Specht that the way from psychiatry to psychology must necessarily pass through philosophy, receives a peculiar illustration in M.'s essay, and it makes one ask seriously: Is it not chiefly a warning against *mere* philosophical method and against too much awe of accepted philosophical rules that is needed in this field?

IV

Oswald Külpe, who deplores the lack of support for psychology as an independent science and department in German universities, offers a practical contribution in an article entitled "Psychologie u. Medizin" in the second number of the *Zeitschrift* (Vol. I., pp. 187-267). He brings very pertinent criticisms of methods and results in recent studies in psychopathology by Isserlin, Binet et Simon, Oesterreich, Liepmann's study of flight of ideas, the problem of mind-blindness, and a program for the examination of mind-blindness. It shows what a wealth of detail is suggested by the systematic and critical consideration of the introspective material. It also shows the distance between the naïve medical attempts and the searching demands of the psychologist who is experienced in what we might call psychohistology; but also the interesting limitation of non-mental issues in his considerations.

Another study of a more concrete character is that of Pick, who presents a most interesting case, showing the rôle of the relation of perception to the self—a patient who went through attacks in which he saw the city as he used to know it and experienced peculiar panics during the conflict of the hallucinations and the real perception of the city as it stood before him, when he could bring himself to help his vision by touch or by rapping a gate or a building with his stick. From very interesting records of the introspection of the patient he shows how important in the conflict between obsessive visual memories or hallucinations and real perception of the real outside world the activations of other sense-mechanisms (and we might also add the motor mechanisms) become.

These studies plainly concern themselves with only a limited field of what psychology will meet in abnormal situations. They do not come out plainly as a new and exclusive method, but as more

conscientious and well-considered efforts to do justice to introspective material, such as must also appeal to one who strives for precision in a non-dogmatic attitude concerning psychology and its relation to science. The last two years have however emphasized more pronounced types of reëmanicipation of psychology into a strictly autonomous position.

One of the ablest German psychologists, Narziss Ach, of Königsberg, says at the end of a discourse "Ueber den Willen": "Die experimentelle Psychologie selbst ist zwar keine Naturwissenschaft, denn den Gegenstand ihrer Untersuchung bilden die geistigen Vorgänge, aber sie benutzt naturwissenschaftliche Methoden zur Festlegung der Gesetze dieser geistigen Vorgänge, etc."

This is the German version of the attitude most emphatically voiced by Yerkes in this country. In his analysis of the replies of physiologists and biologists on the relation of psychology to biology (*J. of Philos., Psychol., etc.*, I, 113-124) and in his *Introduction to Psychology*, Yerkes feels forced to assert what I might call an overcorrection of the ordinary training which tends to disqualify the average person for a naïve and direct use of the introspective material of experience. Yerkes, from conviction or for didactic reasons, is intent on making the student cultivate the view of a world divided into two distinct aspects, psychics and physics; to both of which he can apply the fundamental methods of the natural sciences. Observation under natural and experimental conditions, quantitatively accurate and verifiable description, and causal explanation.

In my own words, without a radical division of psychics and physics, psychology, in the normal, or the abnormal, aims at sufficient differentiative description of these events, determination of the conditions under which they arise and the conditions the events lead to and the law of their modifiability; in other words a reduction to experimental terms or to experimental function. Psychology as a study of events would then be opposed to the consideration of merely logical or at least adynamic relations, and will have to deal also with non-introspective material. Any such effort necessarily presupposes sufficiently organized living beings in action, and as long as the reactions have dynamic factors involved, they can be studied in terms of experiments (not merely in experimental situations); if the dynamic factors are eliminated, a chiefly logical treatment begins, hence the view maintained by Münsterberg that psychology knows only of purposive relations, while the causal chains are observable only in the physical side of the events, which, of course, leaves psychology, as he conceives it, in clear opposition to *Naturwissenschaft*.

It is of course conceivable to think of the states of various degrees of abnormal synthesis in the same terms as of those of normal and not further conditioned mental life, purely in terms of relational and logical sequences or in the mode of psychologizing of common-sense when it leaves outside of the sphere of discussion the non-mental series of physiological or biological events or integrated material. One might go so far as to make sure that the differences in the time-relationships which evidently characterize the different physiological states in a general way, should be expressed in terms of "psychological time" and create a picture of a world of pure introspection. This might be a natural extreme of consistency for the psychologist who enters psychology from the philosophical camp; but for one who approaches it from the field of direct objective as well as subjective experience, without going through the phase of "secondary naïvety" of the philosophical standpoint, this temptation is assigned a secondary place and the emphasis is put upon whatever standpoint gives us the best definition of the conditions under which the matters under discussion occur, and under which we can study them as modifiable factors of experiments.

I cannot help referring here to an exaggerated instance of a pathopsychological study, the quaint book of M. Herz (*Kritische Psychiatrie. Kantische Studien über die Störungen der reinen speculativen Vernunft*. Wien, 1895). Oesterreich's *Phaenomenologie des Ich* would form a more modern type, and also an illustration of a tendency which insists on the "total heterogeneity of psychics as opposed to the processes of external nature."

On the other hand, I am tempted to interweave here a brief abstract of what I should call the direct and naïve call of a physician for a psychology which he can grasp and which is part of his practical and theoretical world. In the main his contentions can readily be compared with Specht's appeal for a psychopathology which recognizes causal chains among and within the mental events.

Koertke (*Somatische Medizin und Psychologie in der Psychiatrie*. Mitteil. aus d. Hamburgischen Staatskrankenanstalten, 1910, 11, 1-17) reviews the uncertainties of psychiatric nosology, cerebral histopathology, the insufficiency of psychological and histological correlations, the improbability of getting far with the mere concept of cerebral localization. He analyzes especially the supposed parallelism between general paralysis and dementia præcox, and the preponderance of a kinship with hysteria and absence of a progressive condition describable in neurological terms in dementia præcox; the need of a

special accounting for the mental symptom-complexes even in general paralysis, and the dreary effects of formal routine diagnoses of dementia præcox which prejudice the physician against the case and throw the patient into the ranks of mere inmates. He urgently calls for a psychological penetration of the cases, points to the advantages of the association-method, not only for the study of hysteria and neurotic states, but for a more intimate knowledge of any case. The psychiatrist cannot afford to be a one-sided physician but must also be a psychologist.

This is in many respects a trend of thought akin to that developed in the decennial lecture at Clark University, or the dynamic interpretation of dementia præcox (*Amer. J. of Psychol.*, 21, 385-403) in which von Voss sees merely Freudian speculations and a one-sided psychological interpretation of the disease, "unintelligible considering the great number of physical manifestations," as if the habit conflicts referred to were one-sidedly psychical unless one expurgates the whole mass of incidental and absolutely intrinsic motor and vasomotor and glandular functions and perversions involved (*Jacobsohn's Jahresbericht*, 14, 1054). Who can blame a critic who only knows the orthodox tenets of a psychology without a body?

V

The appearance of pathopsychology is a somewhat surprising secession and declaration of independence of psychology in the field in which the possibility of a blending of natural science and psychology into a really unitary science had seemed most promising. With all the recognition of discontinuity and pluralism which cold-blooded criticism has to face in a systematic account of experience, there is no doubt about convergence and growing harmony of perspectives in the direction of experimentation as the fundamental trait of modern science. It is not the *essence* of the objects of study but their *sufficient differentiation and what they do*, i. e., the rôle in chains of cause and effect, that goes at the core of what we long for. For a long time psychology had to assert itself against aggressive and sometimes brutally crude types of materialism, and the safest way *was* the complete retirement behind a partition furnished by the parallelistic dogma. Today psychology is in great demand to fill gaps which the coarse materialism has failed to touch, and it is called upon to bring lawful order into our knowledge of and experience with a great and important field of human interests, not only abstract introspection,

but the world of conduct and behavior. Those who approach it from biology and pathology naturally bring a strong interest in the conditions on which certain reactions depend for their very occurrence. Some of these conditions lie in the outside world, others in the organism of the individual and for some of the conditions we can point to states or events expressible only in terms of the symbols characteristic of conscious or mental reactions. The question then arises whether the biologically trained workers must be told that they either must change their faith or remain in the cold world of "matter," excluded from "psychics," or whether they are allowed to take with them the interests in the objective as well as in the subjective data of the science of conduct and behavior, and the privilege of using them according to the accuracy needed or the connection in which the facts appear in the chains of cause and effect. It may be that for reasons of simplicity we should favor the categorical separation of the facts by the maintenance of a rigorous parallelism. But if the simplicity leads to undue restrictions or to undesirable licenses of imagination such as the neurologizing tautologies criticized in previous articles, we are willing to accept more complex rules of procedure and a freer range to our common-sense way of dealing with conduct and behavior and its metabolism or inner mechanisms manifested in introspection and its exteriorization. Instead of dividing the world of facts and of workers into a long series of contrasting types, we specify the rules of the special procedures and keep them subordinated to the main facts and interests without any partitions such as the cumbersome division of the world of experience into psychics and physics, or into pathopsychology and psychopathology, and similar contrasts.

Psychology as the science of conduct and behavior and its mechanisms as a natural science and branch of biology, deals with that range of facts which can in many conditions leave the substrata or the strata of lower integration as "taken for granted," or as sufficiently considered, as long as the data of the psychological strata and of the external stimuli are taken conscientiously. But as soon as we leave the simplest situations, as in the variations of the mental level in conditions of fatigue, sleep, intoxications, brain-disease or even emotional conflicts, etc., it is absolutely necessary to command the facts concerning the entire integrative material, mental and non-mental. Facts which also can be studied apart, as functions of independent organs (such as the brain) or mechanism (such as the polyglandular system of regulations), must then be open to study from the point of view of the broader functions of the individual as a whole, and he

will be the conqueror who commands the whole hierarchy of sciences—from physics and chemistry to the biological sciences (including the science of conduct and behavior) and to logic or the science of relations; from the dynamic sciences with its world of cause and effect to the philosophy of which Specht and Münsterberg say that it is needed to pass from psychiatry to psychology—or we might say, to the philosophy which reflects the manner in which a worker passes from psychiatry to psychology. For it does seem after all as if the connection of the two called for a recasting of the rules of intellectual procedure and a recasting of important assumptions, too often shirked under the excuse of aversion to metaphysics, *i. e.*, to the formulation of systematized logical consequences. Whether or not experience with psychopathology will ever touch or throw any light on fundamental psychological conceptions can safely be left to the future and to the workers who have to shape their fundamental conceptions in keeping with growing experience.

EXPERIMENTAL PSYCHOPATHOLOGY

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Much has been written during the past few years concerning the mutual relations of psychology and medical science. The establishment of laboratories of psychology in hospitals for the insane, in institutions for the feeble-minded, and in universities for the examination of abnormal children has resulted in a wider and more general appreciation of the possible advantages which may accrue to both psychology and medicine by the combination. This interest has been shown to some extent by various attempts to give names to each new application or junction of psychology with one of the medical disciplines, and at present we have to deal with abnormal psychology, psychopathology, pathopsychology and clinical psychology. Many other divisions are made, and the extremes to which this attempted division may go is well illustrated in Wallin's article (14), where we find the terms "clinical psychology," "psycho-clinical," "medical psychology," and "medico-clinical" as well as psychopathology. In many instances the reader is left to judge whether or not each term is to designate something different from the others.

A practical distinction which may be made and held to is that when an investigator is concerned chiefly with the general course of a

disease and its treatment his interests are in psychiatry, but when his chief concern is the investigation of the development or interrelations of mental symptoms his interests are in psychology, and the emphasis, either on the psychological or the pathological aspect, makes his work either pathopsychological or psychopathological. This distinction is well brought out in the work of Gregor (5). In this book Gregor, like his predecessor Störing, gives a general account of the mental processes in a variety of diseases. Although it can not be said to be a complete exposition of all forms of abnormal mental conditions, the book gives a better view of the present status of psychopathology than any other single work. Here one may find a summary of many of the experimental results in psychopathology which are scattered throughout psychiatric and psychological journals, but there is a Germanic exclusiveness which slightly mars the work as a whole. Many of the chapters dealing with psychopathology have companion chapters dealing with the normal psychology of the processes under consideration which serve to bridge the gap between normal psychology and psychopathology and also to introduce the physician to general and experimental psychology. The material included in the book is mostly two or more years old, so that our review precludes the possibility of much more than mention of the titles of the chapters, which are as follows: psychology and psychiatry; psychopathology of time sense; reaction experiments; pathology of apprehension; association reactions; association experiments with the insane; methods of testing memory; pathology of memory; psychology of evidence; experiments on the psychology of evidence of the insane; psychology and pathology of attention; methods of testing attention; experimental testing of movement; bodily changes accompanying mental states; mental work; methods of testing general intelligence.

A notable omission is that of the sensory equipment of the insane, but this defect is not due solely to the author but to the great body of those who investigate the abnormal. It is strange that although psychologists have devoted much time to the investigation of sensation, little or no work of this character has been published regarding the sensory equipment of the abnormal. Psychiatrists have dealt with conduct (or movement, if you will) to the exclusion of sensation except in as far as the latter topic bears directly upon hallucinations and illusions. They criticize the psychologists for their analytic sensation work and demand the investigation of "conduct," showing thereby a lack of appreciation of the fact that "conduct" is a complex depending, in part at least, upon sensations.

In some respects the book of Whipple is an equally notable contribution to general psychopathology (15). Here psychiatrists may find details of more exact methods of testing patients than have usually been employed by them, but which for them have hitherto not been available in simple form, or which have been grouped in college text-books or scattered through many psychological journals. The partial limitation of the object of the book to the study of children prevents a full consideration of it from a psychiatric (*i. e.*, psychopathological) point of view, but many of the methods should prove useful to those who wish to examine the mental states of the insane in ways more exact than those usually employed. From personal experience, the writer is inclined to doubt the psychiatric (*i. e.*, the psychopathological) value of some of the methods advocated, and certain matters have not been taken up which have great value for the psychologically inclined psychiatrist. Many of the methods can be used with the insane only as research methods, others are very simple and of great practical value but are parts of the general equipment of those who have to deal with the insane, *e. g.*, tests for heterophoria *et al.* Because the general character of the tests which are recommended is simple, Whipple's book is much more useful to those who deal with the insane than most other works dealing with experimental method, and in this connection mention may be made of the report of the special committee of the American Psychological Association on the standardization of experimental procedure in tests (12). The committee apparently had in view the application of the methods they advocate solely to the normal. Few of the methods recommended are useful in the examination of the insane, and it is to be regretted that the part of the committee which has already reported has apparently neglected to deal with certain practical relations of psychology and has restricted its report largely to the consideration of the testing of normal individuals. Two of the topics discussed in the report may, it is true, have only a limited bearing upon problems of psychopathology, but that of mental imagery may be important in the consideration of the types of reaction, of hallucinations, or delusions, etc., of the abnormal.

Although, as indicated above, experimental psychologists have devoted a large part of their time to the investigation of sensations, Gregor's work passes over sensation disturbances, and Whipple's book fails to give indications of methods of testing some of the sensations which at times are much altered in the insane and other abnormal classes. Thus, we find no discussion of methods of testing taste,

smell, temperature and the threshold of touch. It may also be noted that in general Whipple's tests of movement are directed towards the testing of motor equipment as such, rather than to the sensations of movement, which are also often disturbed in pathological conditions, not only in the insane but also in the feeble-minded. Recent personal work, not yet published, indicates that the sensory equipment of the insane and of other abnormal classes must be investigated as of equal importance to the motor or conduct sphere, and that there are as many sensory disturbances or deviations in the abnormal as there are motor or conduct disturbances. It is surprising that psychiatrists and psychopathologists have not investigated the sensory equipment of their patients, but part of their failure to do so may be due to the fact that they have been unable to obtain from the normal psychologists data suitable to compare with their own. It is largely because of the necessity of having data on untrained subjects that some psychopathologists have been compelled to devote a large part of their time to experiments on normal, but psychologically untrained, subjects so that a direct comparison with similar results on abnormal, but equally psychologically, untrained subjects may be made. This is what the writer has been compelled to do in his work on the sensations mediated through the skin and the underlying tissues (3), for he has found no available data for purposes of comparison. The methods can usually not be as fine as those used in a purely psychological research, in other words they must be clinical. The results from this work may be little different from those on trained subjects with finer methods, and perhaps no great amount of material for theoretical psychology may accrue from the work, but it is needed for purposes of comparison as practical psychological standards. If we are to have much advance in our understanding of the abnormal and any advance in the understanding of the normal from the study of the abnormal, many tests must be devised and applied to a number of normal, but untrained, subjects and the same tests applied to the numerous abnormal classes. For example, the usual procedures of reaction time experiments can be applied to only a very small percentage of the abnormal. Sommer's tridimensional analyzer can be used with normal subjects but can not be used with many abnormal. Much simpler instruments and methods may be devised to make tests of a similar character and have wide applicability and give valuable results.

Largely on account of the value association tests have for diagnosis, the number of researches on the association of ideas in the

insane is more than on any other topic. Some of the more important of these are worthy of even more extended consideration than can be given in this review. Of the greatest value is that of Kent and Rosanoff (7). These investigators obtained 100 free associations from each of 1,000 normal subjects and have carefully tabulated the results according to their frequency values, so that the results of any abnormal subject may be directly compared with those of the 1,000 normal subjects. The grouping of the normal reactions resulted in the formulation of a table, or tables, of actual facts without the extended consideration of the logical characters of the reactions, as has been done by many previous investigators. Since this review is concerned mainly with pathological advances and methods, we must pass over the normal results and consider only the results on the 250 insane patients. The results on 108 cases of dementia præcox showed a larger number of "individual" reactions than the normal or than any other form of insanity studied; of 33 cases of paranoic conditions, a heterogeneous group, many showed no departure from the normal, and only a few cases closely allied to the dementia præcox group gave evidence of great abnormality; 24 cases of epilepsy showed many repetitions and many particles of speech as association reactions, and it is worthy of note that these cases were mostly in a state of advanced dementia; 32 cases of paresis gave varying reactions, those "presenting no considerable dementia or confusion and cases in a state of remission" gave practically normal reactions, and those showing mental deterioration showed many repetitions, associations to previous reactions, etc.; 32 cases of manic-depressive insanity showed slight variations from the normal, although there was a number of "sound reactions, word complements, and particles"; in 8 cases of involutional melancholia no evident abnormality was observed; 6 cases of alcoholic dementia showed no evidence of abnormality; and only one of the 4 cases of senile dementia showed more than the usual number of individual reactions.

In this connection the works of Klepper (9), of Kilian (8) and of Nathan (10) deserve mention. Klepper investigated the associations of epileptics and katatonics, which types of cases sometimes have a somewhat similar symptomatology and which are, therefore, difficult to differentiate. The characters of the associations differ in the two types which were investigated. Without going into the enumeration of the logical differences in the types of reactions it is evident that there are sufficiently well marked differences, and these are so great that the author concludes that he is able to differentiate one type

from the other by the association tests alone, without having any history or case record. Kilian tested the associations of a case of manic-depressive insanity over a period of five months, during which there was a return to the normal condition. He found a gradual decrease in the number of klang and non-understandable reactions, a decrease in a number of perseverations of the associations, but there was a greater tendency to repetition of the stimulus words. Nathan worked on a case of imbecility, investigating principally the so-called senseless reactions, and found that many of these are due to sense impressions obtained or received immediately before or during the course of the experiments, others were due to ideas present in the mind of the subject, which were more or less stable and apparently personal, and some others were reactions to stimulus words given in previous tests. This study is of great psychological interest on account of its analysis of the senseless reactions, for these are more frequent than is commonly believed, and, as the writer has pointed out in another place, they can not be considered to be senseless for the subject, but senseless only as far as the logical beliefs of the experimenter are concerned.

The attempts to explain some symptoms in abnormal cases by tests of the effects of drugs, a method with which the name of Kraepelin is closely associated, have been continued in the Munich clinic. The work of Schnidtmann (13) is an account of an effort to discover the reason for certain prolonged association reactions in certain pathological cases. Seven subjects were given from 40 to 50 c.c. of alcohol, and their associations tested before and after its ingestion. One of the subjects gave shortened times after the taking of the alcohol, and the other six gave normal or lengthened times. The quality of the associations differed in the individual cases, but these are impossible to summarize in a few words. Another series of tests to determine the effects of alcohol had its origin in the Munich laboratory. Göring (4) tested the effect of similar doses of alcohol on muscular force, apprehension, and the ability to add in 18 cases (11 men and 7 women). Preliminary series of tests were made and the testing series were begun 20 minutes after the ingestion of the alcohol. The tests of muscular force were the last of the series and these were usually begun 42 minutes after the taking of the dose of alcohol. All subjects showed less ability to apprehend after the taking of the alcohol, for there were more mistakes; some were unable to add as many figures, although there were marked deviations both up and down; and the muscular force varied, sometimes being

greater and sometimes less after the alcohol. The seven women were given different amounts of alcohol, and it is not possible to make a full comparison with the men, but in general it may be concluded that the women showed more effects from their doses than did the men, and the author believes they are less resistant, probably being less accustomed to the drug. In neither of these two experiments (Göring and Schnidtmann), although valuable in themselves, can it be said that all the precautions were taken that should be taken. Rivers has shown that alcohol when taken and not recognized does not have the marked effect that Kraepelin and his pupils attribute to it, and the excellent method of Rivers, or a similar one which would give as good control, should have been used in these experiments if the results are to be accepted as they stand. No account of Rivers' work has been taken, or at least the later work of Rivers is not mentioned, and since we know from that work how great an influence upon the reactions "knowledge" may have, we are not justified at present in concluding that the results of the work of Schnidtmann and Göring are more than suggestive.

The application of psychological methods to the investigation of therapeutic procedure has been made in the work of Busch and Plaut (2), who investigated the effect of continuous warm baths upon pulse rate, on temperature, on blood-pressure, on muscular force, on associations, on choice reaction time, on apprehension, and on addition ability. Baths of two hours' duration were taken and the effects of these were investigated in relation to the above mentioned processes in 3 normal subjects and 2 hypomaniacal subjects. In general there was a slight increase in temperature, no noticeable change in the pulse rate, and a slight decrease in the blood pressure. The results with the ergograph (muscular force experiments) were varied, sometimes a greater force than normal was obtained, and sometimes the force was less than normal. The choice reaction time was varied but little, if at all; the accuracy of apprehension was increased about 2 per cent. after the bath; the ability to add was also slightly increased; the character of the associations is difficult to estimate and to summarize. All the results are within the normal variation, and since this is so, the effects of prolonged baths on normal and slightly abnormal individuals may be judged to be insignificant. That such therapeutic measures have a quieting effect upon certain excited cases there can be no doubt, and the present work is of value in that it gives a basis for comparison with the more disturbed of the psychiatric cases.

The results of Ranschburg's study of memory (11) are of importance for normal and pathological psychology. The method used in the work is that of word pairs, the subject being given pairs of words in a series, and after the series is completed is given the first word of each pair and asked to supply the second word. Five series were made with 6, 6, 9, 9, and 9 pairs respectively. There were calculated the percentages of words retained immediately, the time of reproduction, the percentages of words retained after 24 hours, and the characters of the mistakes. Normal children reproduced correctly from 75 to 100 per cent. immediately and about 80 per cent. after 24 hours. The average time for the reproduction was 2 sec. for children from 6 to 12 years of age, and 1.2 sec. for those between 12 and 19. There was only about 25 per cent. correct immediate reproduction in the feeble-minded from 6 to 12 years, and only 60 per cent. for those between the ages of 12 and 19; there was a much greater deviation from the normal after 24 hours. The time for reproduction was from 1 to 10 sec. The general paralytics were poor memorizers; only 2 reproduced correctly as much as 75 per cent. immediately, and 19 of the total number averaged only 7 per cent. Fifteen neurasthenics showed normal memory. Although Ranschburg uses his results as indicators for diagnosis and prognosis, this is successful only in certain specially selected cases and in groups, but not for each individual case in any special type of psychosis.

Many tests for the estimation of the general intelligence of abnormal subjects have been devised, and Becker (1) discusses some of these in relation to paranoia and to dementia præcox. The method used by him was a series of questions which called forth observations or statements from the patients. Following are two examples of the type of questions which he used: (1) "Which is heavier, a pound of lead or a pound of feathers?" (2) "Herodotus says: A lioness can bear only one young, because at its birth the cub destroys the womb of the lioness. Why is this statement false?" Results of these tests can be interpreted only in an indirect fashion, and at times no interpretation is possible. Much depends upon the previous education and training of the individual subject and much more upon his coöperation in the test. At the same time such tests can be used only for large groups if they are intended to have any diagnostic value.

Numerous insane patients show no reaction to stimuli, and casual observation would tend to lead to the conclusion that the stimuli were not apprehended. Some of these cases return to a more normal

condition and can recount much that occurred during the period when they did not respond. The stimuli were apprehended, but the reactions were inhibited. That these patients may appreciate stimuli has been shown by the galvanic reactions obtained from some of them by Wells and Forbes (16). One of their cases of catatonic stupor "showed no evidence whatever of consciousness," but reacted galvanically to all forms of stimuli which were applied. One case of senile dementia showed no marked deflections, which would indicate that the stimuli had been appreciated.

Miss Kent's work on the formation of simple habits in cases of dementia præcox (6) is of great interest for it gives a scientific basis for the work of training of these cases which has been lacking. It is well known that many of these patients may be made very useful about an institution, but there are large numbers which are not trained because it appears on the surface that it would take too long a time to get them to acquire proper habits of work. The results of this work, however, show that it is a comparatively easy matter to get almost any case of dementia præcox trained to perform simple series of movements which are useful. Some of these patients who are normally (sic) destructive and filthy may be taught such movements that the old destructiveness and filthy habits are replaced. One of the most important variables in the work was the coöperation of the subjects, but the tests which were used were of such a character that they were not directly appealing to the subjects and they could not be taken as the best possible conditions for the production of coöperation. In general the curves of training resemble those of animals and the method used by the subjects were mostly those of trial and error, although in certain cases the methods were unlike those of animals and those of normal subjects. Continuation of this work, especially in regard to the factors influencing the method of work, are urgently needed, both for psychopathology and for its applications in psychiatry. Here should come tests of the effects of punishments, and of rewards.

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CONSCIOUS AND UNCONSCIOUS MENTATION FROM THE PSYCHOANALYTIC VIEWPOINT

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The recent writings of Freud and Jung have a special interest and importance because of their concern with the biological foundations on which the Freudian psychology rests. We are led back to the secure ground of first principles from which we may set out anew to reconstruct with the raw materials of primal, rudimentary psychogenic processes.

Freud rallies attention first of all to the element of disunity which is the central and inseparable factor in the production of the neuroses. It is this element of disaffection in the life of the individual—this inherent revulsion to the existence of things as they actually are

and the substitution of a fanciful portrait of things as they might have been—which constitutes the basis of the neurotic diathesis.

A neurosis presupposes then to a greater or a less degree a withdrawal from the world of reality. It becomes in its essence a sinister evasion of actual issues. Janet recognized this hall-mark of these disorders and aptly characterized it as the loss of the "fonction du réel." It remained to Freud to trace the phenomenon to its genetic source. Through his psychoanalytic researches Freud has brought to light the existence of the essential factor in the causation of this recessive, infolding tendency. He has unearthed a hidden *wherefore* in the situation. Abandoning the prevailing static, mechanically deterministic conception of neurotic disorders, he has removed these processes from the place they had formerly occupied in the sphere of brain disease states and placed them upon a dynamic, conative, biologically purposive footing.

Positing the existence of a primary matrix of unconscious processes as the background of mental life, Freud describes it as consisting throughout of a homogeneous pleasure-pain principle (Lust-Unlustprinzip) or briefly pleasure-principle.¹ It is precisely this "pleasure-principle" belonging to the embryonic psychic organism and actuating it to resist the invasions of outer actuality which represents the point of departure in Freud's psychoanalytic method of interpretation. The mechanism whereby the primitive, elemental organism seeks to preserve its even tenor and to repel the encroachments of reality Freud has called the process of repression (Verdrängungsprozess). This repressive mechanism with its entail of conflicts is the central point in Freud's psychological system.

Upon the assumption of this primary, fundamental, self-sufficing pleasure-principle to which the claims of reality are essentially opposed we come to feel the justification of such conceptions as appear to regard neurotic conditions in the light of a commodity rather than an affliction. Accordingly are made light usages which were dark; such for example as the reference Freud makes to "the choice of a neurosis" or "the retreat into the psychosis" wherein, contrary to the prevailing view, these disorders are represented as rather of the nature of a deliverance than a disease.

Freud regards this pleasure-principle then as primary. It is the elemental psychic principle, constituting, as it were, the menstruum of consciousness. Originally in a state of quiescence its equilibrium

¹ FREUD, S., Formulierungen ueber die zwei Prinzipien des psychischen Geschehens. *Jahrbuch für psychoanalyt. u. psychopathol. Forschungen*, III.

is first disturbed through the functional demands of the organism. "In this case," says Freud, "what was thought (*i. e.*, wished) was simply hallucinated, just as happens later nightly in our dreams." It is only when the required satisfaction is no longer to be had through the process of hallucination, that the psychic organism is forced to yield to the importunities of reality. Thus is thrust in upon the psyche the recognition of a stern outer reality in contradistinction to the benign inner world of fancy. And thus is introduced over against the primal pleasure-principle the principle of reality.

With the entrance of the reality-principle a whole system of adaptations is demanded of the psychic organism—adaptations with which we are familiar in the various activities of consciousness; as for example the adjustments of attention and observation; memory; the purposeful motor innervations; and the higher intellectual processes. Meanwhile there is preserved still the original pleasure-principle which in its antagonism to the reality-principle has established a mode of psychic activity all its own—*das Phantasieren* or phantastic thinking "which begins with the play of children and, continued later as day-dreams, tends to release our contact with objective reality."

The gradual infringement of the elements of the reality-principle upon the pleasure-principle does not take place uniformly throughout. This is notably illustrated in regard to the sexual trends which are tardiest in the process of transformation. So that while the other trends of the ego are responding in a measure to the behests of the reality-principle, the sexual trend, remaining in arrears, continues still under the sway of the primary pleasure-principle. Thus sexuality being in the beginning autoerotic tends to remain in this phase, and, because of the possibilities it is afforded in the direction of autoerotic satisfactions, the sexual trend is in consequence bound up for a proportionately longer time with the pleasure-principle; in which phase indeed it is in many individuals, through the process of repression, delayed throughout life. In consequence of these relations there is established a closer connection between the sexual trend and the sphere of the phantastic on the one hand and the remaining trends of the ego and the conscious activities on the other. "In the sphere of the phantastic, repression remains supreme; so that it comes to pass that images in *statu nascendi*, if their cognition can give rise to a painful affect, are blocked before they may reach consciousness."

It follows then that an essential part of the psychic disposition to the neurosis lies in the retarded evolution of the sexual trend in its rela-

tion to reality. The pleasure-ego desires merely the immediate satisfaction, however transient and unstable; while the reality-ego seeks the ultimate and permanent good. The former seeks satisfaction in chimerical illusions, the latter in scientific reality. So that education is in truth nothing else than the progressive displacement of the pleasure- by the reality-principle.

As an interruption (through unconscious repression) in the course of readaptation of these two basic trends—the egoistic and libidinous—may occur at any stage of the developmental process, it follows that the character of the resulting neurosis is dependent upon or rather is concomitant with the (unconscious) choice of the phase of retardation and that therefore the character of a neurosis should be studied in relation to the genetic mode in which the above-mentioned developmental arrest takes place.

In the chapter "Ueber die zwei Arten des Denkens"¹ of his "Wandlungen und Symbole der Libido" Jung enters upon a more detailed discussion of the theme unfolded by Freud in his paper on the "Zwei Prinzipien," namely, that of the essential distinction between the characters of the psychological processes involved in conscious and in unconscious mentation.

Setting out with the empirically manifest phenomenon of symbolization presented in dreams, Jung enters directly *in medias res* with the pertinent psychological inquiry as to "whence it comes that dreams are symbolic." The more dynamic problem involved in the question "wherefore are dreams symbolic?" is left aside because involving issues no less extensive than the Freudian system of psychology itself.

He first calls attention to the characteristic absence of symbolism in the type of psychic activity we know as conscious thinking, such psychic processes for example as are brought to bear upon the solution of a given problem, and says how upon scrutiny it becomes manifest that this species of mental activity invariably depends upon verbal imagery—that in reality words, or their motor equivalents, are the indispensable medium of thought.

Thinking then, *i. e.*, directed, purposeful thinking, tends to expression, to communicable form. It seeks to address itself outwardly and to conform to reality; in other words it tends to reflect "the succession of objectively real things." So that biologically stated conscious thinking, like every vital function, is an adaptation to environment.

¹ JUNG, C. G., Wandlungen und Symbole der Libido. *Jahrbuch für psychoanalyt. u. psychopathol. Forschungen*, III.

Jung recalls to mind the biological rudiments of speech which consist "of a system of emotional and imitative sounds" as attested today in the onomatopœic vestiges of current usage. "So that speech is originally and essentially nothing else than a system of signs and symbols which indicate real processes or their reverberation in the human soul" and "howsoever abstract a system of philosophy, it yet represents in regard to end and means nothing else than the most highly elaborated combination of primordial sounds."

Adaptation to the natural sequence of outward phenomena—imitation of and conformity to reality—is characteristic of conscious, directed, verbal thinking. It is the progressive, social, externally assimilable type of psychic activity.

Contrariwise, thinking which is not conscious proceeds aimlessly, intransitively, unproductively. Its flow runs without fixed, predetermined course. It is restricted by no anterior design. It is subjective and automatic, image succeeding image in passive obedience to unpremeditated quests. Unconscious thinking being unpurposeful soon leads away from reality into phantasies of past and future. It does not reflect things as they are but decks them out in fanciful array. Thus it represents what is *wished* in contradistinction to what *is*. Because of its likeness to the psychic processes familiar to us in the phantasmagoria of sleep, it is the custom to give to this manner of thinking the name of "dreams."

Comparably with all phenomena in the scale of evolution these two types of psychic activity have their ethnic as well as their individual aspect—conscious thinking being represented in its ethnic phase in the practical system of organized scientific ratiocinations characteristic of our own adult age, while *das Phantasieren* is represented in the phantastic, bizarre, mythological vagaries through which the childhood of the race was wont to seek appeasement. Thus the forward, scientific trend of thought of the present age is the phylogenetic correlate of individual consciousness as presented in the ontogenetic series and correspondingly the illusory, visionary, unreal constructions belonging to the psychic infancy of the race find their genetic analogy in the phantastic, "play" creations characteristic of the psychic life of individual childhood. The analogy here indicated is nothing else than the mental concomitance of the familiar correspondence stage for stage in the historic development of individual characters, as shown in the comparative study of anatomical and embryological evolution. "The myth," says Karl Abraham,¹ "is a

¹ ABRAHAM, KARL. *Traum und Mythos*. 1909.

vestigial remnant of the psychic infancy of the race and the dream is the myth of the individual." But in the phantasies entering into the psychic life of the normal individual of modern times there is lack of conscious indorsement, while the phantasies belonging to an antique cultural period were elevated to a conscious social plane and given the significance of national credence. This is especially illustrated in the phantasies occurring within the sexual sphere. "The symbolism relating to the instrument of coitus was an inexhaustible topic for the fancies of antiquity." So that there arose extensive cults of phallic worshippers. The phallic symbols appeared in countless forms, *e. g.*, as the bird, the fish, the snake, etc., and there existed national theriomorphic representations of the sexual trend, comparable to the theriomorphic symbols of sexuality which the psychoanalyst meets anew in the dreams of the neurotic.

"Viewed from this standpoint, the symbolism which Freud has discovered is seen to be an expression (limited to the dream, the symptom-act and to mental aberrations) of thought processes and psychobiological trends which once exerted a most powerful influence over past cultural epochs."

The type of thinking we call unconscious, uncontrolled, subjective, tending, as it does, to elude the rigid causality of outer reality, is therefore essentially *infantile*, for it belongs to the infancy of the individual and of the race.

"It would seem then that the psyche possesses an historical stratification in which the oldest strata correspond to the unconscious." So that when in later life there occurs an introversion (in the sense of Jung), it consists of a harking back to regressive, reminiscent, infantile material of the individual's (ontogenetic) past, but when a yet further regression takes place (as in the introversion psychosis—skizophrenia) "there are presented outspoken traces of an archaic mentality which under circumstances can extend backward even to the revivification of psychic processes which have now become wholly archaic."

The philosophical discussion the trend of which is here but briefly indicated is the preamble to a detailed analysis of the unconscious material presented in a publication of a Miss Frank Miller under the title "*Quelques faits d'imagination creatrice subconsciente*," in which Jung traces the thread of unconscious symbolism running through them and points out the interesting correlation between the symbolisms in Miss Miller's *poésie* and the symbolism contained in the legends of an early mythology. To follow the author into the inter-

esting inductions he draws from the analysis of Miss Miller's picturesque phantasy would be however to infringe the limits of the present review.

THE PRESENT STATUS OF THE BINET SCALE OF TESTS FOR THE MEASUREMENT OF INTELLIGENCE

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In 1905, Professor Binet published in *l'Année psychologique* (4) a tentative scale of some thirty tests for the measurement of intelligence, these tests being arranged in order of difficulty, beginning with the very simplest kinds of mental adaptation. In *l'Année* for 1908 (5), Binet and Simon published the new and much improved scale which has become the basis of world-wide discussion. The tests of this scale, about fifty in number, had been carefully applied to some two hundred normal Paris children of the poorer quarter, as well as to many defective children. There are from three to eight tests for each year from three to thirteen inclusive, and these groups of tests are given as norms for children of these ages and of corresponding social and industrial rank. The scale appeared almost simultaneously with a law providing for the establishment of special classes for defective children in the schools of France. In a little volume, *Les Enfants Anormaux* (3), Binet and Simon give illustrative directions for the use of the scale in selecting the children to be placed in special classes.

In April, 1911, these authors published (6) a revision of the scale embodying the results of its further application by themselves and others. Five tests are assigned to each year, except for the fourth year. A test was ranked as normal to a given age when 75 per cent. of the children of that age passed it. Above the ten-year level, tests are not given for other levels than 12 years, 15 years, and "adult" (used for "above 15 years").

In *l'Année* for 1911 (2), Binet and Simon present this revised scale with an extended discussion of their experiments and of the criticisms that have been made of the scale, particularly by experimenters in Belgium, England, and America. This article is worthy of note as being Prof. Binet's final word about the scale before his death, which occurred in October, 1911. It seems due to these authors that we sketch here their own very candid review of the "Binet literature":

Decroly and Degand (8) applied the 1908 scale to 43 normal girls and boys in a private school in Brussels. The parents were physicians, lawyers, professors, etc., of very much higher station than the poor Parisian working-people. The classes were small (8-10 pupils), and the instruction highly individualized. Binet and Simon obtained these experimenters' notes of their tests and review them in detail. The Belgians were found to be more indulgent than the French in giving the tests. The Belgian pupils tested in advance in the tests requiring attention, language, and "family lessons," and were behind in the six tests which depend partly on "school exercises." On the average they tested a year and a half in advance of the Paris children, which in Binet's opinion illustrates the difference in intelligence level and in language between children of the poor and of the rich. The Decroly and Degand results do not therefore call for a revision of the scale. Tests in Paris show a decidedly higher level of intelligence ($\frac{3}{4}$ year), in children of a well-to-do quarter over those in a poor quarter, though this difference does not hold for rich and poor taken from the same school.

Miss Katherine Johnston (14) applied the tests to two hundred pupils of the schools in Sheffield, England. Binet and Simon examined the notes of these tests. They find that the tests were applied to some schools of the rich and to others of the extremely poor, and that "these heterogeneous results are confounded in the averages." Irregular variations from the Paris norms are thus to be expected, as found, according as the children were of different social and industrial stations. The count was not made according to the method proper to the scale. Properly counted, and allowing for the admitted fact that in the 1908 scale "the tests for 11 and 12 years are much too severe," Binet and Simon conclude that "the results of Miss Johnston are in perfect accord with ours."

Binet and Simon do not accept the opinion of Whipple (24) and others that the tests are too easy. They consider that Whipple's substitutes for the supposedly "cruel" nonsense statements "cannot be accepted before being tried experimentally. There is nothing to prove that they present a difficulty of comprehension equal to that of our own." The "cruelty" of these sentences does not affect the Paris children unfavorably. They "laugh at them."

Binet and Simon urge as a lesson that they have long been learning and as profoundly important, that intelligence and other mental functions are to be measured by what children do for a *variety* of different tests and not for any one. When correlations of intelligence

with other functions are to be determined, it is these *resultants* that are to be dealt with. "A particular test, isolated from all the rest, is not worth much," and is "subject to errors of all sorts," especially if it is rapid. One could almost say, "the tests matter little if they are only numerous enough." For almost any series of tests the number to which replies are satisfactory "grows regularly enough with age."

As to the general employment of the scale by teachers, these authors call the scale a "*Méthode de luxe*," which like the vernier and microscope secure a fineness of estimate not necessary for most teachers, who are too busy to employ it and do not need to employ it. It is a method which requires "*apprentissage*," to be employed, like the microscope, in "*une étude soigneuse*."

Leaving at this point the review by Binet and Simon, we find that the tests have had much further discussion, of which but partial report can be made in this article.

Whipple includes in his Manual (24) descriptions of the 1905 and 1908 scales with directions for their use, and summarizes the criticisms of Decroly and Degand.

Goddard (9, 10) applied the 1908 scale throughout the New Jersey Training School for feeble-minded children, at Vineland, and finds that results for the 400 children "agree perfectly with long experience in institution life," and a second testing "shows remarkable agreement with the first." Goddard's tests of 1,547 normal children in the first six grades show that the largest number test just to their age, while successively smaller numbers test to higher or lower age levels, these numbers arranging themselves in a normal curve of distribution. (Criticism of this curve, by Terman and by Ayres, will be noted later.) Seventy-eight per cent. of these children test to their age or within a year above or below it. However, certain tests seemed to be wrongly placed, particularly for the latest years; and in view of these and other tests of normal children and of Binet's own revision, Goddard has published (11) a revised scale on the general plan of Binet's revision, but giving tests for 11 years and placing some of the other tests differently. After one year Goddard re-tested 1,000 of the normal children, and states that the results "show considerable correlation with the earlier test, but with marked and peculiar differences which must be explained." He finds that "feeble-minded children tested from two to seven times show remarkable uniformity in the results, largely regardless of the experience and personnel of the examiner."

Beginning with the autumn of 1909, Huey (13) has used the Binet scale continuously with defectives, at the Illinois state institution at Lincoln and later at the Johns Hopkins Dispensary. The results published for the Lincoln work state that the scale has been indispensable in this actual work with cases, without attempting criticism which the author believes should be based on the examination of normal children. Huey's statement of the scale incorporates the revision of Goddard, and gives the directions most necessary for the employment of the tests.

Wallin (23), as psychologist to the New Jersey State Village for Epileptics, at Skillman, has likewise found the scale a routine necessity in his examinations, but suggests certain revisions. He prints a complete set of detailed directions for giving the 1908 scale.

Kuhlmann (15) has found the scale of great practical service in his work as psychologist to the Minnesota state institution for the feeble-minded, and has used it in the examination of more than 1,300 children. He publishes a condensed and partial translation of the 1908 scale, from the original article of Binet and Simon. He presents the tests and the directions and comments of their authors, with such adaptations as are necessary for American practice.

Bobertag (7) had already reported the Binet-Simon tests in the *Zeitschrift*. In the article here referred to he publishes the results obtained in applying the 1908 tests to 355 normal children of the schools, of the ages 5 to 12 years; and to 80 *Hilfschule* children of 8-14 years, all in the schools of Breslau. He gives a full and painstaking account of his method of applying each test, with statement of his results, comments, and criticism. He suggests many changes, especially the amelioration, since made by Binet and Simon, of the too severe tests for 11, 12, and 13 years. No one who purposes making a revision of the scale should neglect to review this discussion of the tests by Bobertag.

Lawrence (17) tested 784 public school children, of all the grades from 6 to 13 years, with Binet's *definition* tests (use, superior to use, difference between paper and cloth, etc., meaning of charity, etc., difference between poverty and misery, etc.). The teachers, independently of these tests, sent in estimates of the scholarship of these pupils. In these definition tests, 435 pupils tested to age or not more than a year below; 140 were in advance and 209 were retarded. Seventy-five per cent. of the pupils pointed out by these tests as behind their age are so recognized in the teachers' independent estimate of scholarship, while seventy-nine per cent. of those marked

"good" or "excellent" in scholarship tested to age or above it. Seventy-five per cent. of the school "laggards" "were found by the tests to be mentally retarded one year or more," while the same per cent. of those who are advancing in school more rapidly than the rank and file are found, as above indicated, to be advanced a year or more mentally." The 8-year test was "decidedly too easy" and the "13-year decidedly too hard."

Terman (20), in giving his impressions after testing 90 children and supervising the testing of about 400, says that "by far the most important result was a decided conviction that measuring scales of this general type are feasible, and that when corrected, extended, and multiplied, they will prove of great practical and theoretical value." He finds that "the scale originally offered by Binet is in general far too easy at the lower end, while in the upper ranges it is too difficult. . . . However, in spite of the many imperfections and inadequacies of the revised scale I believe that by its use it is possible for the psychologist to submit, after a 40-minute diagnostication, a more reliable and more enlightening estimate of the child's intelligence than most teachers can offer after a year of daily contact in the school-room." He believes, nevertheless, that "tests of intelligence stand in serious need of further attention before we undertake to determine standards of performance in the different branches of the curriculum."

In a later article Terman and Childs (21) while further eulogizing the plan and usefulness of the scale, urge the need of its revision and extension. Their results, for the tests referred to above, show that on the average their California children of 4-6 years tested to nearly $1\frac{1}{2}$ years above their chronological age, while the children of $11\frac{1}{2}$ to $13\frac{1}{2}$ years tested from one to two years below their chronological age. Goddard's *table* of distribution (10, p. 234) for each age really shows much of this same tendency, particularly for the latest years. Terman considers that Goddard's *curve* of distribution, "lumping all the ages together conceals, of course, the very facts we wish to know. From the above (Goddard's curve) it is seen that the number of younger pupils testing ahead is about balanced by the number of older ones testing behind. What we want to know is how nearly accurate the scale is at every point." Of course it is to be remembered that the 1911 revisions of the scale are in the direction of remedying the errors noted for the later years. Terman and Childs have been trying out some additional tests along with those of Binet and Simon, and plan to publish a revised scale on the basis of their work.

Without having used the scale to any extent with cases, Ayres (1)

presents certain criticisms of it: (1) The tests are largely tests of language ability. (2) Five depend on recent environmental influences. (3) Seven depend on reading and writing. (4) The ability to repeat words and numbers is given too much importance. (5) The same is true of "puzzle tests" and definition of abstract terms. (6) The tests do not sufficiently test native ability, but rather scholastic and other attainments. (7) Due account is not taken of the emotions, habit, etc. He admits that Binet and Simon's "application of tests to a definite, universally understood scale . . . constitutes so important a contribution that its excellence outweighs the shortcomings of the tests themselves." Ayres reaffirms and illustrates Terman's criticism of the fallacy in Goddard's distribution curve. He urges that an improved scale be worked out by coördinating the work of a large number of experimenters, "by some central agency or agencies," to develop a scale that will "really measure native ability."

Kuhlmann in a later article (16), after using the scale in testing 1,300 feeble-minded children, replies *seriatim* to the criticisms of Dr. Ayres, considering most of these to be erroneous and to "come largely from a misunderstanding as to what the different individual tests aim at, and of the mental processes involved in them. The former might have been largely obviated by a more careful consideration of the author's original publications, and the latter by a careful and extensive use of the tests themselves. There is especially a general impression that the authors meant that the results with each individual test will always come out just right, which impression Dr. Ayres seems to share somewhat. If this degree of perfection were attained, only one test of mental age for each chronological age would be necessary, where the authors use from four to eight, and besides point out that this or that individual test often gives wrong results. Probably not a single test in the whole system is free from such objection. In general this article reminds one that it is easy to make criticisms and difficult often to clearly disprove them. But even so, the validity of merely possible objections is not thereby established."

Clara Harrison Town (22), in an especially timely article apropos of the present popularization of the scale, says: "Accustomed to the complicated apparatus of a psychological laboratory, the laity were pleased to find it unnecessary, and overlooked entirely the fact that the psychologist himself was not unnecessary." She quotes Binet's own warnings that "It is not, in spite of appearances, an automatic method, comparable to a scale which, when one stands upon it, throws out a ticket on which one's weight is printed. It is not a

mechanical method, and we predict to the busy physician who wishes to apply it in hospitals, that he will meet with disappointments. The results of our examinations are of no value if they are separated from all commentary; an interpretation is necessary. . . . The idea that a method of examination can be made precise enough to be trusted to every one must be abandoned; all scientific procedure is but an instrument which requires the direction of an intelligent hand. . . . Any one can use it for his personal satisfaction or to obtain an approximation evaluation of the intelligence of a child; but for the result of this method to have a scientific value, it is absolutely necessary that the individual who uses it should have had an apprenticeship in a laboratory of pedagogy or possess a thorough practical knowledge of psychological experimentation."

Dr. Town, who is herself having extensive experience in the use of the scale at the Illinois state institution at Lincoln, reminds us that Dr. Ayres' "whole critique is based on the 1908 series of tests, which has since been revised and greatly altered by Binet himself"; and also that many of his criticisms rest on a misunderstanding of these tests themselves or of the manner in which they are actually given and scored. In general Dr. Town believes that "the result which is threatening is a wholesale use of the scale in an unscientific manner, which will do nothing but postpone the time of its real usefulness—that time when it will be applied by experts along the four practical lines indicated by Binet himself—the grading of normal and backward children in the schools, the diagnosis and classification of abnormal children, the arrangement of school curricula, and in the courts of law."

Meumann (18) makes a condensed but keen analysis of tests of the intelligence, outlining their present status and attempting a constructive interpretation of the principles involved.

Seashore (19) urges briefly that "retardation does not follow a common flat level any more than growth does, nor even nearly so much." We "should not be satisfied with a flat mental age" except for rough classification, as in determining whether a child is feeble-minded. A child may be at the mental age of six in one capacity and twelve in another, and "the important thing to know about the individual is this difference and direction of unsymmetrical development." He thinks the Binet tests should be developed to measure relative rank or age "of more specific capacities and powers, such as reasoning ability, sensory observation, memory, imagination, initiative, emotional control, self-control, etc."

The present writer has elsewhere (12; 13, Chapter VII.) urged such an extension of the principle of the scale to other and more specific mental functions, and believes that this may be a source of most fruitful development in genetic psychology.

In general the scale of Binet and Simon has interested us all in making more methodical study of the intelligence. It has been of immediate and valuable service to psychologists in making examinations of defectives, and it gives promise of being developed to a scale which will render much service in the classification and study of normal pupils. It is hoped that psychologists will prove themselves clinically vigorous enough to use the present scale as a means of growth to the far better ones that Binet himself foresaw.

As for the many non-psychologists who have to make estimates of intelligence, an investigation by Binet himself showed that in making these practical estimates the appeal is regularly to tests, to try-outs of the individual in one or another way. Such persons will at least find the Binet and other series of tests an enrichment of their stock of home-made devices, often economical of time and giving a glimpse, at least, of mental efficiency in more varied and representative directions. Each will make these tests somewhat in his own fashion, inevitably, and the results will by no means match the standardized results. It will be a try-out with a series of test-groups that are progressively more difficult, to see how far the individual can go with them. The more intelligent individuals will be found to go the further, case will be compared with case, and thus if each will give the tests somewhat *uniformly for his own cases*, they may be made the means of building up a more methodical and correct procedure in making estimates of mental efficiency. The tests will not displace the practical judgment, but may be of great assistance in forming and improving it. And even the trained clinical psychologist, with the scale at its best, will doubtless have to "set" it somewhat differently for various social and industrious classes, and will make various allowances for local circumstances, even if not for his own "personal equation."

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SPECIAL REVIEWS

DEMENTIA PRÆCOX

Dementia Praecox oder Gruppe der Schizophrenien. BLEULER. Leipzig und Wien: Franz Deuticke, 1911.

Of a comprehensive work on psychiatry, the "Handbuch der Psychiatrie," edited by Aschaffenburg, there have been published thus far two parts, one on manic-depressive insanity by Stransky, and one on dementia præcox by Bleuler. The latter work, a book of some 400 pages, takes up the study of this important disorder in a very comprehensive manner. The purpose of the present writer is to give here a brief review, not of the clinical-nosological part of the work, but of the psychological portion entirely. It is, however, necessary to state that Bleuler comprises in his book a great many cases which others would not include in the group or groups of dementia præcox, so that his analysis refers in reality a great deal to the symptomatology of the functional psychoses.

What characterizes the manifestations of dementia præcox are: a more or less marked disorder of the train of thought, sometimes spoken of as scattering of ideation with bizarre turns, fragmentary thoughts, and so on; frequently a lack of harmony of affect and intellectual content; an absence of correlation of mental contents; various interferences with the train of thought in the form of sudden stoppage of a topical nature, or of diffuse inhibition of mental operations [termed blocking of thought (*Sperrung*)]; the frequent appearance of impulses opposite to what the circumstances would demand, or a warding off of all interferences, diffuse, or under certain situations only (negativism); a general tendency to shut out the world of reality (by Bleuler called "autism"); an affective deterioration; and finally such symptoms as hallucinations, delusions, impulsive acts, peculiar mannerisms, and the like.

Bleuler gives an excellent and extensive description of all these symptoms, and also makes an attempt at a psychological explanation which he admits frankly to be tentative, yet which to the psychiatrist is of great value and represents a splendid attempt at clarifying the mechanisms in a disorder which, at best, is complex and difficult to understand. Bleuler has proposed to give to dementia præcox the

name schizophrenia, because a great deal in the symptomatology refers to a tearing asunder of normal mental cohesions. This he proceeds to analyze more in detail. He assumes, first of all, a primary diffuse "association disorder," which he seems to consider not further reducible, and which he attributes, therefore, directly to a physical disease process. This he admits to be problematical. The association disorder, therefore, represents an ultimate defect, very much in the same way, I take it, as the loss of memory represents an ultimate defect in the organic disorders. He refrains, however, from any correlation with anatomical changes. This primary disease process he attempts to support by some other phenomena, notably physical ones, an attempt which is not especially convincing. The primary association disorder, he conceives as a sort of leveling down of normal associative affinities, which leads in itself to elisions and fragmentary mental products but which, above all, forms the fundamental defect upon which develop the other symptoms. This is possible, particularly, owing to the fact that the affects can then exert a much greater influence than normally; in other words, the symptomatology is largely determined by the affectivity, through mechanisms, some of which Freud has taught. Bleuler therefore shows how affective complexes are at the bottom of a great deal in the manifestations of dementia præcox, and how, in addition to primary splitting, there is an extensive secondary splitting produced by the affects; thus he admits the great importance of psychogenesis in dementia præcox, but he confines this action to the secondary symptoms, which represent more or less plain evasions of difficult situations.

The psychology of dementia præcox cannot be understood unless we accept the importance of unconscious trains of thought which follow in many ways the same laws as conscious thinking, and which manifest themselves through Freudian mechanisms. But this influence is a much more extensive one in dementia præcox than in the normal, or in hysteria, for example. The affects produce much more profound dissociations. The influence of reality is much more excluded. We might almost say, the individual manifestations stand in the mind much more like foreign bodies. We have attempted to account for this largely by the shut-in tendencies, which probably must be referred to more fundamental defects in make-up and which manifest themselves more or less early in tendencies to live in a world apart where the correcting influence of reality is more or less excluded. Bleuler attributes all this, the shut-in tendencies included, to his primary association disorder, which, as we have said, in turn, gives

the affects greater sway. Whereas the logical train of thought follows paths established by experience, the affects direct the train of thought according to desires and aversions. In the normal they are responsible only for the general direction of action, and the logical operations are not falsified except in realms where subjectivity is generally permitted to guide us, as in matters of taste, for example. In dementia praecox the affects disturb even otherwise well-grounded associations. Through this greater influence of the affects, the possibility of a more or less complete exclusion of all that does not harmonize with the affective complexes is also possible, so that these assume more and more a certain autonomy, and can manifest themselves without there being any attempt at correlation.

The remarkable affective deterioration is, according to Bleuler, a secondary phenomenon. He justly points to the fact that normal affects can be produced in dementia praecox patients when they are forced to think of their complexes; he shows how in the beginning of the disorder, the affective deterioration is by no means general but refers to certain topics only (affective complexes), and also that cases with apparent affective deterioration sometimes get well. Hence he assumes not a loss, but a repression of affects. These repressed affects manifest themselves in various ways but also inhibit other affects. This is not unlike what we see in normal individuals who are preoccupied with an affectful experience. In addition to this, the autism and the splitting off of affective complexes have their share in the production of the general indifference.

The normal individual includes in his logical operations more or less everything in his past and present experience, which has a bearing, irrespective of its emotional value; the fundamental schizophrenic disorder on the other hand makes the exclusion of external and internal facts possible, and permits the natural tendency to live in fancies to flourish. Bleuler speaks of this as autistic thinking, and of the general tendency to turn away from reality as autism, making it dependent, therefore, upon his primary association disorder. In attempting to make negativism comprehensible, he points in the first place, to the fact that every impulse is closely associated with its opposite, which he looks upon as a sort of protective mechanism and which he designates ambivalence. This assumes pathological proportions in dementia praecox. But this negativism, of course, is also closely related to autism and further accentuated by it, and other factors also contribute to the prominence which this symptom may attain in dementia praecox, such as a certain sensitiveness and an instinctive desire on the part of the patient to protect himself against

actual or possible irritation of "mental wounds," also an opposition to the more or less hostile attitude of the environment, and very likely, not infrequently, a certain difficulty in thinking and acting.

What we term "blocking" is undoubtedly an exaggeration of that which we normally know as repression, and whenever the symptom was analyzed in cases, it could be traced to the influences of complexes. But Bleuler also refers some general reductions of activity, or some more or less pronounced conditions of inhibition of mental operations to the same principle, because this can be seen at times to develop out of more topical blocking; and he likens this to the so-called emotive stupor seen in normal persons; at the same time the tendency to stereotypy of impulses, and the tendency to generalization seen in dementia præcox, as well as the lack of interest and the difficulty in mental operations, may also contribute to the full development of this blocking. Stupor or stupor-like reductions may also be due to hallucinations and other causes, *e. g.*, a certain cerebral torpor conceived by Bleuler as the direct outcome of the disease process. Catalepsy, though difficult to explain, seems at least in part accounted for by a certain dearth of ideas which, in other conditions, is also found to be associated with it.

Many symptoms, such as delusion, hallucinations, and odd acts, are direct intrusions into consciousness, of subconscious complexes, and represent wishes and fears, often symbolized and only comprehensible when the possibility of symbolism is fully recognized. They are often difficult to analyze because they may be distorted by substitutions or gradual metamorphoses. The peculiar disharmony between affects and ideas is certainly often explained by the fact that, just as in the case of dreams, the ideas stand for something else than what they appear to represent.

An important part of the work is to be found in the fact that Bleuler clearly demarcates the disorders found in dementia præcox from those of the organic mental disorders, and justly shows that an elementary memory and apprehension defect, as well as primary motility symptoms, are foreign to the symptomatology of dementia præcox.

Such a short review cannot do justice to a work which contains so much that is valuable. A thorough study of it will repay every one interested in normal and abnormal psychology and in the analysis of the most complex pathological phenomena, in which constant reference is made to normal processes.

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